

SIDNEY FARBER CANCER INSTITUTE
CHARLES A. DANA CANCER CENTER
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AUG 22 1977

THE JIMMY FUND

July 20, 1977

Mr. Benno Schmidt
Chairman, President's Cancer Panel
J. H. Whitney and Company
630 Fifth Avenue, Room 3200
New York, New York 10020

Dear Mr. Schmidt:

Thank you for your lengthy reply of May 16 to my earlier letter regarding failure of the National Cancer Institute adequately to support innovative, fundamental cancer research. You primarily provide statistics to defend your statement that basic research is more adequately funded than ever by the N.C.I. You do not really address a number of the main problems that I pointed out. I am responding with the hope that the differences in our points of view can be used constructively to improve and strengthen the national cancer effort. I would also like to take this opportunity to express my appreciation for your major role in obtaining increased funding for this overall effort.

How can it be that your administrative-level overview makes the basic science funding picture so bright for cancer research; but my view is so bleak, from the laboratory and based on discussions with many of this country's best scientists? The difference in part comes from the fact that we are talking about different things. Your "basic science" includes not only investigator initiated grants but also research contracts, support contracts, and center grants. It is the first of these, investigator initiated grants, that comes closest to what I would call truly basic science and which includes the innovative science I wish to distinguish from your more inclusive category of basic science. So long as you apply your statistics to the total basic science picture, they are misleading in regard to how well truly fundamental and innovative cancer research is being funded.

Few people have the native talent and the extensive background (based on hard work) to be truly innovative in science, as is the case for success in almost any human endeavor. The discoveries by these people are the most precious contribution to solving the cancer problem. One top-notch person can contribute what any number of merely competent ones cannot, alone or in committees. Yet everywhere I go I hear the same story: The best scientists, who are recognized as such by their peers, have serious difficulty in being funded. I ask, by analogy, would you as a businessman invest in an oil company that is making small profits, whose wells are going dry, has a very large payroll, and yet is planning to save a few percent of its expenditures by firing its best oil prospectors?

The best and most original cancer research is suffering the most financially. As your statistics show, about 2% of the N.C.I. budget was allocated for new investigator initiated grants! Less than 30% of approved

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new grants were funded! And most of them at only a fraction of the money requested. Less than one-sixth of N.C.I. funds supported investigator-initiated grants in all stages. Even this fraction includes the large sums given to center grants, that do not provide directly for basic research; another large fraction goes to the ever increasing overhead that is not available directly to the investigator. This problem of inadequate funding is compounded by cautiousness it introduces into Study Section and Council decisions as to what applications to fund. I frequently hear of grants not being funded because of fear of taking a moderate degree of "risk." As a consequence, even though the top-rated 30% of approved new grants were funded, I do not believe that they included the best investigators; rather, second-quality grants were often funded because they were "safer."

Thus, although 50% of the N.C.I. funds were allocated to a category called "basic research", a small fraction actually ended up in being allocated to support fundamental, innovative research.

Truly fundamental research is especially important in the cancer area since so few good ideas exist. Without good ideas, the rest is just stumbling around in the dark, hoping to bump into some useful results.

Original research is exploration rather than engineering. It tries to derive new approaches to cancer through pursuit of ideas more than one intellectual step removed from the obvious. No one can see very far in the present situation; but some investigators are less blind than others. The difficulty with convincing others, particularly those who are not scientists, of the value of innovative research is not only, as you state, that individual questions are too small, like pieces of a jigsaw puzzle not yet fitted in; but also they are too many steps removed from the general pattern for most people lacking background knowledge to perceive their significance. The present funding methods of the N.C.I. fail particularly in this distinguishing between second-class and first-class science. Indeed, it is usually the best science that suffers, because it is too far ahead to be easily comprehended by the N.C.I. administrators. I ask you as a test to compare the inverse ratio between the quality of Institutions and their contract/grant funding.

Funding of top quality investigator initiated grants should come first rather than last in N.C.I. budget planning, and its year-to-year stability should be planned. How much more money would be needed to fund all investigator initiated proposals receiving priorities of "very good" (cutoff at priority 2.5)? I would guess from your statistics that a few tens of millions of dollars would make up the difference, for both new grants and renewals. This is far less than the amount you surmised on pages 7 and 8 of the President's Panel Report (April 15), in which you said that it would require the entire N.C.I. budget.

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You express surprise at my negative statements about in-house research and say that it is excellent. Certainly some of it is excellent. But some of it is very bad and much is inbetween, just as in the scientific world as a whole. This holds true as well for center grants and research contracts. Is it a good expenditure of U.S. money to cut off individual grants with high priority ratings, and yet fund in-house work and contracts down to a much lower quality level?

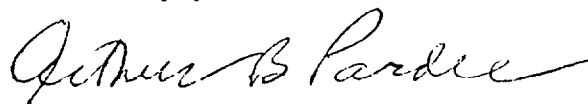
From where could this money come? From other sectors of the N.C.I. budget. The weakest statements in your letter have to do with the support contracts (\$90 million), the products of which go to relatively few people who, as far as I can make out, are not particularly distinguished but who have enormous total budgets, both direct and as a consequence of these support contracts. I suspect that the Zinder Report attacked this problem, though I have not seen this report (Could you have a copy sent to me?)

I am not at all persuaded that the best expenditure of American taxpayers' dollars is for a small number of second-rate problems that employ numerous third-rate investigators and use supply contract materials made by an army of technicians. These "ideas" are the warmed-over generalizations of committees or managers who are often inadequate scientists at best. The tone-deaf do not select music; why should the idea-deaf select research?

The truly basic research is not the only activity that the N.C.I. must support, but I do believe it is the most important. Those who advocate even more support for efforts that will provide a "more immediate payoff" should look at the past rate of progress. Without fundamental research the cancer effort is about as useful as a golden razor with a nicked blade.

A number of my friends tell me that they and others have received copies of your letter to me. They should be equally interested in my response, and so may I ask that you send copies of this letter to the same mailing list to whom you sent your letter to me?

Sincerely yours,

A handwritten signature in cursive script, reading "Arthur B. Pardee".

Arthur B. Pardee
Professor

ABP/ds